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Effects of Worked Examples, Example-Problem, and Problem-Example Pairs on Novices' Learning

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Abstract

Research has demonstrated that instruction that relies more heavily on example study is more effective for novices' learning than instruction consisting of problem solving. However, 'a heavier reliance on example study' has been implemented in different ways. For example, worked examples only (WE), example-problem pairs (WE-PS), or problem-example pairs (PS-WE) have been used. This study investigated the effectiveness of all three strategies compared to problem solving only (PS), using electrical circuits troubleshooting tasks; participants were secondary education students who were novices concerning those tasks. Based on prior research, it was hypothesized and confirmed that WE and WE-PS would lead to lower cognitive load during learning and higher learning outcomes than PS. In addition, the open questions of whether there would be any differences between WE and WE-PS, and whether there would be any differences between PS-WE and PS were explored. Results showed no differences between WE and WE-PS or between PS-WE and PS. This study can inform instructional designers on which example-based learning strategies to implement: it does not seem necessary to alternate example study and problem solving, but when doing so, example-problem pairs should be used rather than problem-example pairs.

Effects of Worked Examples, Example-Problem, and Problem-Example Pairs on Novices' Learning

Whereas conventional problems contain only a description of "givens" (e.g., how fast a car accelerates and its average velocity) along with a goal statement (e.g., 'calculate how far the car has travelled'), worked examples additionally show learners the worked-out solution steps required to reach the goal. Research has shown that for novices, instruction that relies more heavily on worked example study is generally more effective for learning and transfer than instruction consisting of problem solving, and is also often more efficient, in that this higher learning is reached with less investment of time or mental effort (for reviews, see Atkinson, Derry, Renkl, & Wortham, 2000; Renkl, 2005; Sweller, Van Merriënboer, & Paas, 1998; Van Gog & Rummel, 2010). This is known as the 'worked example effect', which cognitive load theory has explained in terms of the different cognitive processes evoked by problem solving and example study (Sweller, 1988; Sweller et al., 1998).

Cognitive Load Theory and the Worked Example Effect

Cognitive load theory (Sweller, 1988; Sweller et al., 1998) is concerned with the design of instructional methods that efficiently use people's limited cognitive processing capacity. The theory distinguishes three types of cognitive load: Intrinsic, extraneous, and germane load. Intrinsic load depends on the complexity of a task, that is, on the number of interacting information elements the task contains (Chandler & Sweller, 1991). These elements have to be processed simultaneously in order to successfully learn to perform that kind of task. Because working memory capacity is limited to seven plus or minus two elements (or chunks) of information when merely holding information (Miller, 1956) and even fewer (ca. four) when processing information (Cowan, 2001), the higher this number of interacting information elements is, the higher the intrinsic load imposed on working memory is. However, intrinsic load does not solely depend on the nature of the task, but also on the level of expertise of the learner. As expertise increases, information elements contained in the task become incorporated into cognitive schemata stored in long-term memory. Because a schema retrieved from long-term memory can be handled in working memory as a single element, the number of interacting information elements a task contains, and hence, the intrinsic load it imposes, decreases for more knowledgeable learners. With extended practice, certain schemata can be automated and no longer require controlled, effortful processing, which further reduces the load on working memory (Schneider & Shiffrin, 1977).

Extraneous and germane load both depend on the way the task is designed, with extraneous load being imposed by processes that are evoked by the design of the task that are ineffective for learning (e.g., unnecessary visual or mental search processes; Chandler & Sweller, 1991) and germane load being imposed by processes evoked by the design of the task that do contribute to learning (e.g., imagining a solution procedure; Cooper, Tindall-Ford, Chandler, & Sweller, 2001). The central tenet of cognitive load theory is that in order to be effective and efficient, instruction should be designed in such a way that intrinsic load is optimized, that is, tasks should be at an appropriate level of complexity for trainees, extraneous load is minimized, and germane load is optimized so that the available cognitive capacity is optimally used (Sweller et al., 1998). An effective instructional technique to accomplish this is to implement a heavier reliance on worked example study rather than problem solving.

Instruction that consists mainly of solving conventional problems forces novices to resort to weak problem-solving strategies such as means-ends analysis, in which learners continuously search for operators to reduce the difference between the current problem state and the goal state (Sweller, 1988). This imposes a high extraneous load on working memory, and is not effective for learning: Even though such weak strategies may allow learners to succeed in solving the problem eventually (i.e., *performance*), they have been shown to contribute very little to *learning*, that is, to building a cognitive schema of how such problems should be solved. Worked examples prevent the use of such weak problem-solving strategies, allowing the learner instead to devote all the available cognitive capacity to studying the worked-out solution procedure (i.e., the relationship between problem states and operators) and constructing a cognitive schema for solving such problems (Sweller & Cooper, 1985). Such schemas can go beyond the specific problem-solving procedure that was shown: general rules can be abstracted from the examples (e.g., Anderson & Fincham, 1994; Anderson, Fincham, & Douglas, 1997), which enables students not only to solve similar (retention) problems, but also transfer problems for which (part of) the solution procedure has to be adapted (see e.g., Cooper & Sweller, 1987; Paas, 1992; Paas & Van Merriënboer, 1994).

It should be noted that the worked example effect is found only when the examples are well-designed and do not, for instance, induce split-attention or provide redundant information (e.g., Tarmizi & Sweller, 1988). In addition, it is found mainly for novice learners. For students who have higher levels of prior knowledge of a task, the intrinsic load imposed by a task is lower, because task elements have been incorporated into a cognitive schema that can guide their problem solving. Therefore, students with higher levels of prior knowledge no longer need the guidance provided by worked examples, and for them, example study can be ineffective or even detrimental for learning compared to problem solving (Kalyuga, Chandler, Tuovinen, & Sweller, 2001; Kalyuga & Sweller, 2004).

With well-designed examples, the worked example effect has been demonstrated to occur for novice learners in a variety of domains. Earlier studies were mainly conducted with highly structured tasks such as algebra (e.g., Carroll, 1994; Cooper & Sweller, 1987; Sweller & Cooper, 1985), statistics (Paas, 1992; Quilici & Mayer, 1996), geometry (e.g., Paas & Van Merriënboer, 1994; Tarmizi & Sweller, 1988), or physics (e.g., Van Gog, Paas, & Van Merriënboer, 2006; Ward & Sweller, 1990), but recent studies have shown that the worked example effect is also found with less structured tasks such as learning to recognize designer styles (Rourke & Sweller, 2009). In addition, it has also been demonstrated in *collaborative* learning situations (Rummel & Spada, 2005; Rummel, Spada, & Hauser, 2009). Research on the worked example effect has been criticized for using problem solving without any instructional support whatsoever (i.e., no assistance) as a control condition (Koedinger & Aleven, 2007). Recent studies have shown, however, that a heavier reliance on worked examples can also enhance learning, reduce acquisition time, or both, compared to *tutored* problem solving, which is a control condition that provides students with more assistance (e.g., feedback, hints) than conventional problem-solving (McLaren, Lim, & Koedinger, 2008; Salden, Aleven, Renkl, & Schwonke, 2009; Schwonke, Renkl, Krieg, Wittwer, Aleven, & Salden, 2009).

Different Example-Based Learning Strategies

Placing more emphasis on example study during instruction can be done in different ways. A few studies have compared the effects of *example study only* to problem solving only, and found example study to be more effective for learning and transfer as well as more efficient in terms of mental effort investment (Van Gerven, Paas, Van Merriënboer, & Schmidt, 2002; Van Gog et al., 2006). Most studies, however, have *alternated* example study with problem solving. Several studies have shown that *example-problem pairs* were more effective for learning and transfer than problem solving only (Carroll, 1994; Cooper & Sweller, 1987; Kalyuga et al., 2001; Mwangi & Sweller, 1998; Rourke & Sweller, 2009; Sweller & Cooper, 1985). Sweller and Cooper (1985) mention that engaging in solving a similar problem immediately after example study may be more motivating for students, because it allows for more activity than studying another example would. Trafton and Reiser (1993) compared example-problem pairs to a condition in which students first studied four examples and then solved four problems, and found the example-problem pairs to be more effective. It has not yet been investigated, however, whether example-problem pairs would be more effective than examples only.

A few studies have investigated the use of *problem-example pairs*, arguing that when learners first experience deficiencies in their performance during problem solving, they may be more motivated to study the example and may focus on the steps that they could not solve (e.g., Hausmann, Van de Sande, & VanLehn, 2008; Reisslein, Atkinson, Seeling, and Reisslein, 2006; Stark, Gruber, Renkl, & Mandel, 2000). It is questionable, however, whether novice learners are able to accurately diagnose their own performance deficiencies (for a review, see Bjork, 1999).

The studies by Paas (1992) and Paas and Van Merriënboer (1994) are interesting in this respect. Their problem solving conditions were given a worked example of the same problem as feedback when they did not succeed in solving a problem within a certain time or certain number of attempts. So the example was *identical* to the problem students had just attempted to solve, which would allow students to pay attention to the exact steps that proved to be problematic for them. Nevertheless, the examples conditions were more effective than the problem solving condition in which examples were given as feedback when the student could not solve the problem. This suggests that those novice learners were not able to use the examples as feedback effectively, presumably because they were not able to accurately diagnose their own performance deficiencies. The ability to accurately self-assess performance seems to be related to one’s knowledge of the tasks (Dunning, Johnson, Erlinger, & Kruger, 2003), which novices lack. This fits with the findings of Reisslein et al. (2006), who found an interaction of example-problem pairs and problem-example pairs with learners’ prior knowledge: Whereas low prior knowledge learners benefited most from example-problem pairs, high prior knowledge learners benefited most from problem-example pairs.

The effects on novices’ cognitive load and learning of problem-example pairs have not yet been compared to problem solving only. Hausman et al. compared the elaboration processes occurring during problem-example pairs and tutored problem solving, but they did not implement a pre-posttest design, so no information on effects on learning is available.

Even though many of the above mentioned studies were inspired by cognitive load theory (Sweller et al., 1998) not all of them addressed the effects of cognitive load imposed by the different example-based learning strategies. Measuring cognitive load, for example via subjective mental effort rating scales (Paas, 1992; for an in-depth discussion, see Paas, Tuovinen, Tabbers, & Van Gerven, 2003), can reveal important information for researchers that is not necessarily reflected by traditional performance-based measures such as accuracy or the number or type of errors made. Particularly, the combination of performance and cognitive load measures can provide information concerning the relative efficiency of instructional methods, both in terms of the learning process and in terms of learning outcomes (see Paas & Van Merriënboer, 1993; Van Gog & Paas, 2008). In terms of the learning process, instructional methods that impose less cognitive load during the learning phase but result in equal or higher learning outcomes (i.e., performance during a subsequent test phase), can be called more efficient. In terms of learning outcomes, a higher test performance combined with lower or equal (respectively) cognitive load imposed by those test tasks, shows that more efficient cognitive schemata have been acquired. Some studies in which mental effort was measured, have shown example-based learning strategies to be more efficient than problem solving both in terms of the learning process (e.g., Paas & Van Merriënboer, 1994; Van Gog et al., 2006) and in terms of learning outcomes (e.g., Paas, 1992; Paas & Van Merriënboer, 1994).

The Present Study

Despite the substantial amount of research that has been conducted on example-based learning, no studies exist that included all four conditions; comparing the effects of examples only, example-problem pairs, problem-example pairs, and problem solving only on novices' cognitive load and learning. Doing so can provide important information to instructional designers regarding the relative effectiveness of the different strategies. Based on previous research, it can be hypothesized that examples only and example-problem pairs will be more effective and efficient than problem solving only for novices. In addition, important open questions that this study seeks to answer are whether problem-example pairs will be more effective and/or efficient than problem solving only, and whether example-problem pairs are more effective and/or efficient than example study only. Regarding the first question, the effects on learning from problem-example pairs have not yet been compared to learning from problem solving only. On the one hand, problem-example pairs do provide learners with more information than problem solving only, and moreover, when learners first experience deficiencies in their performance during problem solving, they may be more motivated to study the example and may focus on the steps that they could not solve. On the other hand, as mentioned above, studies in which examples were used as feedback in the problem solving condition do not seem to provide much support for this assumption. Regarding the second question, example-problem pairs might on the one hand be more motivating for novices than example study only, because they allow them to actively apply what they have just learned. Although this sounds plausible, there is no empirical research available yet to support this assumption.

Method

Participants and Design

Participants were 103 secondary education students from two Dutch schools (48 male; age *M* = 16.22, *SD* = 0.84). They were in their fourth or fifth year of pre-university education (the highest level of secondary education in the Netherlands, which has a six year duration). Participants were assumed to be novices concerning this kind of task. Some of them did take science classes, and would, at this stage in their curriculum, be familiar with Ohm's law. However, they had no experience in their curriculum with applying Ohm's law to reason about faults in electrical circuits, which was the task we used in this experiment (see materials section for more details). Participants were randomly assigned to one of the four conditions: (1) problem solving only (*n* = 26), (2) problem-example pairs (*n* = 26), (3) example-problem pairs (*n* = 25), and (4) example study only (*n* = 26).

Materials

Prior knowledge test. The prior knowledge test consisted of seven open-ended questions on troubleshooting and parallel circuits principles. This test had been developed for and used in a previous study (Van Gog, Paas, & Van Merriënboer, 2008) in collaboration with two secondary education science teachers. Examples of items are "What do you know about the total current in parallel circuits?", "What is probably going on if you do not measure any current in a parallel branch of the circuit?".

Training tasks. The two pairs of training tasks (i.e., four tasks in total) consisted of a malfunctioning parallel electrical circuit presented on paper that participants had to 'troubleshoot' (i.e., diagnose the fault). These tasks were almost identical to the tasks used in previous studies by the Van Gog et al. (2006, 2008), which were developed in collaboration with two secondary education science teachers, with the difference that the tasks in the present study did not require interaction with an electrical circuits computer simulation program.

In the circuit drawing (see Figure 1) it was indicated how much voltage the power source delivered and how much resistance each resistor provided. Based on this information, participants first had to calculate the current they would expect to measure in each of the parallel branches as well as overall. Then, they were given measurements that were incorrect supposing the circuit would function normally. Based on these measurements, they could infer that something was malfunctioning, and they were asked to indicate what the fault in the circuit was. In the first pair of tasks, the fault was that *lower* current was measured in a particular parallel branch, which is indicative of *higher* resistance in that branch. In the second pair of tasks, the fault was that *higher* current was measured in a particular parallel branch, which is indicative of *lower* resistance in that branch.

When tasks were presented in problem format, the circuit drawing was given along with the questions participants had to answer: (1) 'Determine how this circuit should function using Ohm's law, that is, determine what the current is that you should measure at the ammeters?', (2) (this was given) 'Suppose the ammeters indicate the following measurements: ….', (3) 'What is the fault and in which component is it located?'. In the example format, the solutions to questions 1 and 3 were already worked-out and students had to study the solution procedure (see Figure 2). The order of the four tasks was kept constant across conditions, only the format of each task varied between conditions (problem or example).

Test tasks. The test tasks consisted of two problems. One was highly similar to the training tasks, consisting of a parallel circuit with a same kind of fault as the training tasks (i.e., too low total current caused by too low current in one parallel branch indicating too high resistance in that branch), but the circuit drawing and the values of components differed from the training tasks. The other problem also consisted of a parallel circuit that contained the same kind of fault as the training tasks, however, it contained two faults rather than one and those could not be inferred from the total current (i.e., total current was as it should be, but resulted from current in one parallel branch being too low and current in another parallel branch being too high, indicating too high and too low resistance in those branches, respectively).

Formula sheet. On one page A4 paper, Ohm's law was explained and the different forms of the formula were given (i.e., R=U/I; U=R*I; I=U/R).

Mental effort rating scale. The 9-point mental effort rating scale developed by Paas (1992) was used, which asked participants to rate how much mental effort they invested in studying the preceding example or solving the preceding problem, with answer options ranging from (1) "very, very low mental effort" to (9) "very, very high mental effort". This scale is widely used in educational research (for overviews, see Paas et al., 2003; Van Gog & Paas, 2008). According to Paas et al. (2003), mental effort reflects the actual cognitive load, that is, the cognitive capacity that is allocated by the individual to accommodate the demands imposed by the task.

Procedure

The experiment was run in four group sessions of approximately 30 min. duration at participants' schools. Within each session, participants were randomly assigned to one of the four conditions. Participants first received some general information about the experimental procedure: what parts the experiment consisted of, what the general sequence and time keeping procedure was, and that they were allowed to use their calculator (the experimenters provided calculators for those students who did not bring their own). Then, they completed the prior knowledge test (5 min.). After the prior knowledge test, participants worked on the training tasks associated with their condition. These were provided in a booklet, with a mental effort rating scale inserted after each task. Each task and each mental effort rating scale was printed on a separate page of A4 paper. Participants were orally instructed to complete the mental effort scale after they had finished the task and then to wait for a sign of the experimenter before proceeding to the next task. They were reminded of this each time they completed a task or mental effort scale by means of printed instructions below the task or scale. Participants were given maximally 3 minutes per task. This amount of time was considered to be sufficient to solve the problem or study the example based on data from previous studies (Van Gog et al., 2006, 2008) in which students had to interact with an electrical circuits computer simulation program (i.e., the tasks in the present experiment would take less time than those in the previous experiments). Time was kept by the experimenter using a stopwatch, and the experimenter indicated when learners were allowed to proceed to the next task. Participants had been instructed to perform the tasks sequentially, and were not allowed to look back at previous tasks or look ahead to the next task in order to prevent the example-problem pairs and problem-example pairs conditions from using the examples during problem solving. An experimenter and one or two associates kept surveillance to discourage students from trying to look back or ahead. Participants were allowed to use the 'formula sheet' and a calculator. After the training tasks were completed, the experimenter and associates collected the booklets with the training tasks so that participants could not look back at those while working on the test tasks. While collecting the training materials, they also checked whether participants had not written on their 'formula sheet' and replaced it if necessary. Participants then completed the two test tasks, which were also presented in a booklet interspersed with mental effort rating scales on separate pages, and the procedure was identical to that used with the training tasks.

Data Analysis

The maximum total score on the prior knowledge test was 10 points. For the test task with only one fault, a maximum score of 3 points could be gained: One point for correctly calculating the current at all ammeters, one point for correctly indicating the faulty component, and one point for indicating what the fault was (i.e., what the actual resistance was). For the test task containing two faults, a maximum score of 5 points could be gained:

One point for correctly calculating the current at all ammeters, one point per component (i.e., max. two points) for correctly indicating a faulty component, and one point per fault (i.e., max. two points) for correctly indicating what the fault was in each component (i.e., what the actual resistance was). Because the scoring procedure for the prior knowledge test was based on a model answer sheet that had been previously developed and used in another study (Van Gog et al., 2008) and the scoring procedure for the test tasks was straightforward and did not leave much room for interpretation, scoring was done by one of the authors, who was not aware of participants' experimental condition.

Results

In total, seven participants had missing values on the test tasks and were therefore excluded from the analysis. Four of them were in the problem-problem pairs and three of them in the example-problem pairs (3) condition (leaving $n = 22$ in those conditions). The mean performance and mental effort data per condition are shown in Table 1. When prior knowledge is taken into account as a covariate in the analyses, the adjusted means and standard errors show the same pattern. Checks on distribution of the data showed that values for skewness and kurtosis fell within the cut-off valued defined by Kline (1998) of – to + 3 for skewness and – to + 8 for kurtosis: skewness values ranged from -0.1 to 0.4, and kurtosis values ranged from -0.3 to 1.0.

A univariate analysis of variance on the mean mental effort invested in the training tasks with prior knowledge as covariate, showed a significant difference between conditions, $F(3,91) = 7.78$, $MSE = 3.48$, $p < .001$, $\eta_p^2 = .20$. Bonferroni post-hoc tests indicated that invested mental effort was significantly lower in the examples only condition than in the problems only ($p < .05$) and problem-example pairs conditions ($p < .01$), and that invested mental effort was significantly lower in the example-problem pairs condition compared to the problems only ($p < .05$) and problem-example pairs conditions ($p < .01$). The examples only and example-problem pairs conditions did not differ from each other ($p = 1$), nor did the problems only and problem-example pairs conditions ($p = 1$). In sum, invested mental effort in the training tasks was significantly lower in the examples only and example-problem pairs conditions than in the problems only and problem-example pairs conditions.

A multivariate analysis of variance on test performance and mean mental effort invested in the test tasks with prior knowledge as covariate, showed a significant difference between conditions, Pillai's trace = .342, $F(6,182) = 6.26$, $p < .001$, $\eta_p^2 = .17$. The subsequent univariate analyses were significant for both test performance, $F(3,91) = 9.00$, $MSE = 4.69$, $p < .001$, $\eta_p^2 = .23$ and mean mental effort invested in the test tasks, $F(3,91) = 5.03$, $MSE = 4.73$, $p < .01$, $\eta_p^2 = .14$. Bonferroni post-hoc tests indicated that performance on the test tasks was significantly higher in the examples only condition than in the problems only ($p < .05$) and problem-example pairs conditions ($p < .01$), and that performance on the test tasks was significantly higher in the example-problem pairs condition compared to the problems only ($p < .05$) and problem-example pairs conditions ($p < .01$). The examples only and example-problem pairs conditions did not differ from each other on test performance ($p = 1$), nor did the problems only and problem-example pairs conditions ($p = 1$). In sum, performance on the test tasks was significantly higher in the examples only and example-problem pairs conditions than in the problems only and problem-example pairs conditions. The post-hoc tests also indicated that mental effort invested in the test tasks was significantly lower in the example-problem pairs condition than in the problem-example pairs condition ($p < .01$), and marginally lower in the examples only condition than in the problem-example pairs condition ($p = .064$). All other differences were not significant (all $p > .05$).

Discussion

This study aimed to compare the effects of example study only, example-problem pairs, problem-example pairs, and problem solving on novices' cognitive load and learning.

Results showed that the problem solving only and problem-example pairs conditions were less effective than the examples only and example-problem pairs conditions. Not only did the examples only and the example-problem pairs conditions significantly outperform the problem solving only and problem-example pairs conditions on the test, this higher performance was also reached with significantly *lower* investment of mental effort during the training. This is indicative of higher efficiency in terms of the learning process, that is, in terms of the cognitive ‘costs’ and benefits of training (see Van Gog & Paas, 2008).

Despite suggestions that allowing students to apply what they have just learned from the example might be more motivating and better for learning (e.g., Sweller & Cooper, 1985; Trafton & Reiser, 1993), example-problem pairs were not more effective or efficient than examples only. It should be noted though, that a limitation of this study is that the amount of tasks in both the training and the test phase was rather low. It is possible that positive effects on motivation and learning of example-problem pairs compared to examples only would occur on longer training phases.

This study also takes away a potential criticism (e.g., Koedinger & Aleven, 2007) of previous studies on the worked example effect in which examples only (e.g., Van Gerven et al., 2002; Van Gog et al., 2006) or example-problem pairs (e.g., Cooper & Sweller, 1987; Kalyuga et al., 2001; Sweller & Cooper, 1985) were typically compared to problem solving only: that the benefit over problem solving stems only from the fact that the example conditions received more information. That is, participants in examples conditions can study how such problems have to be solved, whereas the problem-solving condition usually gets no such information (with the exception of some studies, e.g., Paas, 1992; Paas & Van Merriënboer, 1994). However, our study showed that these conditions also did better than the problem-example pairs condition, and this condition received exactly the same amount of information as the example-problem pairs condition. This finding both supports and refines the cognitive load theory explanation of the worked example effect. Sweller et al. (1998) state: “Studying worked examples also eliminates means-ends search, and so a heavy use of worked examples as a substitute for solving problems may be also beneficial. In contrast to conventional problems, worked examples focus attention on problem states and associated operators (i.e., solution steps), enabling learners to induce generalized solutions or schemas.” (pp. 273). Our results show that merely substituting some of the practice problems with worked examples is not necessarily effective, but rather, that effectiveness depends on when these examples are provided: before or after problem solving. The finding that examples are beneficial when provided before problems fits the cognitive load theory view that worked examples allow students to acquire cognitive schemas that can guide future problem solving.

The question remains, however, why an example following a problem solving attempt does not have the same effect. There are at least three (not mutually exclusive) possible explanations: 1) students may not study the example as closely as they should because they fail to recognize the deficiencies in their performance, 2) students may not be motivated to study the example because of the negative experience of a failed problem solving attempt, or 3) students may be prone to hindsight bias (Bjork, 1999), that is, upon seeing the solution procedure they might incorrectly believe that they would in principle have been capable of solving the problem correctly. Regarding the first explanation: research has shown that novices are very often unable to accurately assess their performance, as this ability seems to rely (at least partly) on knowledge about the task (Dunning et al., 2003), so the question is whether novices would be able to recognize exactly where the deficiencies in their problem solving lie (as Stark and colleagues, 2000, suggested) and to which steps they would therefore need to pay especially close attention during examples study. Students might need some prior knowledge to be able to do that. The findings by Reisslein et al. (2006) support this view: they showed that for learners with more prior knowledge, problem-example pairs may be more effective than example-problem pairs. However, this does not explain why, if they would not be able to determine what steps in the example they should pay special attention to, students did not just study the whole example closely. This suggests there is some motivational effect or a cognitive process such as hindsight bias at work.

To uncover exactly what metacognitive or motivational processes are at work when learners are provided with problem-example pairs, as well as other combinations of examples and problems, future research might use process-tracing techniques such as concurrent or (cued) retrospective verbal reports (Ericsson & Simon, 1993; Van Gog, Paas, Van Merriënboer, & Witte, 2005). In addition to documenting the processes involved in learning from different combinations of examples and problems in more detail, it would be interesting for future research in this area to use longer training and test phases with different ratios of examples and problems to analyze changes in learning over an extended time period during which students acquire prior knowledge (i.e., for learners with high prior knowledge, problem-solving is known to become equally if not more effective than studying worked examples or example-problem pairs; Kalyuga et al., 2001; for a review, see Kalyuga, Ayres, Chandler, & Sweller, 2003).

In sum, this study shows that it is not strictly necessary to alternate example study and problem solving; example study only and example-problem pairs were equally effective and efficient. If example study and problem solving is alternated, however, example-problem pairs should be used, because problem-example pairs did not lead to better learning than problem solving only.

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Table 1

Mean (and SD) Prior Knowledge, Test Performance, and Mental Effort Scores per Condition

	Problem-Problem		Problem-Example		Example-Problem		Example-Example	
	(*n* = 22)		(*n* = 26)		(*n* = 22)		(*n* = 26)	
	M	*SD*	*M*	*SD*	*M*	*SD*	*M*	*SD*
Prior Knowledge (max. 10)	3.43	1.67	2.90	1.45	2.84	1.79	3.00	1.38
Mental Effort Training Phase (max. 9)	6.49	1.79	6.76	2.09	4.85	1.85	4.92	2.10
Performance Test Tasks (max. 8)	2.66	1.58	2.48	2.31	4.70	2.78	4.75	2.56
Mean Mental Effort Test Tasks (max. 9)	5.59	2.46	6.94	2.08	4.61	2.54	5.29	2.11

Figure 1. Example of a circuit. *Note*: AM1,2,3,4 = ammeters; R1,2,3 = resistors (the number behind each resistor indicates its resistance in Ohm), V = power source (5 Volts), SW = switch.

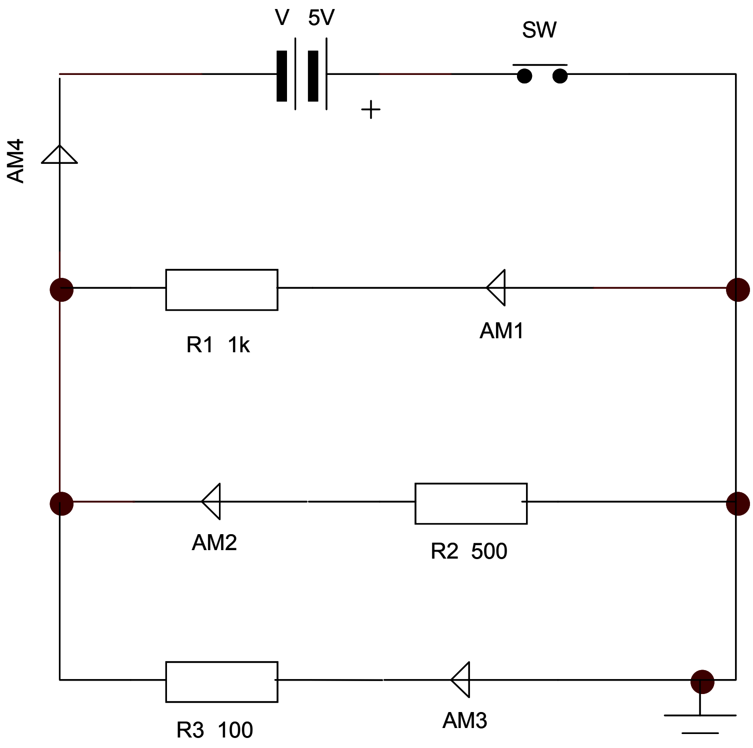

Figure 2. Worked example of the circuit shown in Figure 1 (a small version of the circuit diagrams was always shown in the upper right corner of the page in all examples and problems). In the training problems and the test tasks, students had to answer questions 1 and 3 themselves.

1. Determine how this circuit <u>should</u> function using Ohm's law, that is, determine what the current is that you should measure at AM1 to AM4

In parallel circuits, the total current (I_t) equals the sum of the currents in the parallel branches (I_1, I_2, etc.).

The total current should be: $I_t = I_1 + I_2 + I_3$

or:

$$I_t = \frac{U}{R_1}+\frac{U}{R_2}+\frac{U}{R_3}=\frac{5V}{1k\Omega}+\frac{5V}{500\Omega}+\frac{5V}{100\Omega}=5mA+10mA+50mA=65mA$$

This means you should measure:

AM1 = 5*mA* AM2 = 10*mA* AM3 = 50*mA* Am4 = 65*mA*

2. Suppose the ammeters indicate the following measurements:

AM1 = 5*mA* AM2 = 7,14*mA* AM3 = 50*mA* AM4 = 62,14*mA*

In this case, the calculation of what you should measure does not correspond to the actual measures, so something is wrong in this circuit.

3. What is the fault and in which component is it located?

If the current in a branch is lower than it should be, the resistance in that branch is higher
(equal U divided by higher R results in lower I).

The current in the second branch is smaller than it should be: I_2 = 7,14*mA* instead of 10*mA*. Thus, R_2 has a higher resistance than the indicated 500Ω. The actual resistance of R_2 can be calculated using the <u>measured</u> current:

$$R_2 = \frac{U}{I_2} = \frac{5V}{7{,}14mA} = 0{,}7k\Omega = 700\Omega$$